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ABSTRACT

Random assignment experiments are widely used in the United States to test the effectiveness of new social interventions. This paper discusses several major welfare-to-work experiments, highlighting their evaluation from simple "black box" tests of single interventions to multi-group designs used to compare alternative interventions or to isolate the effects of components of an intervention. The paper also discusses new efforts to combine experimental and nonexperimental analyses in order to test underlying program theories and maximize the knowledge gained about the effectiveness of social programs. Researchers and policymakers in Britain may find this variety of approaches useful to consider as they debate an expanded role for social experiments. (Contains 46 references.) (Author/SLD)

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MDRC Working Papers on Research Methodology

# Extending the Reach of Randomized Social Experiments: New Directions in Evaluations of American Welfare-to-Work and Employment Initiatives

James A. Riccio
Howard S. Bloom

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*ABSTRACT*

Random assignment experiments are widely used in the United States to test the effectiveness of new social interventions. This paper discusses several major welfare-to-work experiments, highlighting their evolution from simple "black box" tests of single interventions to multi-group designs used to compare alternative interventions or to isolate the effects of components of an intervention. The paper also discusses new efforts to combine experimental and non-experimental analyses in order to test underlying program theories and maximize the knowledge gained about the effectiveness of social programs. Researchers and policymakers in Britain may find this variety of approaches useful to consider as they debate an expanded role for social experiments.

## Introduction

During the last three decades, social experiments have been used extensively in the United States to determine the effectiveness of social programs and have played an especially prominent role in the evaluation of welfare-to-work and employment programs (Friedlander, Greenberg, and Robins, 1997; Greenberg and Shroder, 1997). This fact is a testament to the widely held belief that such experiments, which employ random assignment to allocate subjects to treatment and control groups, are the best way to produce unbiased estimates of program effects (e.g., Betsey, Hollister, and Papageorgiou, 1985; Boruch, 1997). It is also a testament to the growing recognition that in many settings it is possible to conduct random assignment on a large scale in a fair and ethical manner (Boruch, 1997; Gueron, 1999).

Random assignment to a new treatment is generally accepted as ethical when the effectiveness of the treatment is unknown, when subjects for the experiment are not being denied access to an opportunity to which they are legally entitled, and when resources are not sufficient to provide access for everyone who might qualify. In this situation, random assignment can be viewed as a fair way of allocating scarce resources, because it gives everyone involved the same chance to receive them.

However, random assignment is not always technically feasible, even when the political will to use it exists. In some cases, the nature of the intervention (for example, interventions focused on whole groups) makes it impractical to choose individual subjects on a random basis. In other cases, random assignment is too blunt an instrument for testing important program theories or hypotheses. Thus, although it has proven to be a powerful tool for assessing social policies, random assignment is often limited by technical and practical constraints.

The reality of these constraints poses a challenge for social researchers: how to work within and around them in order to maximize the value of social experiments for knowledge development. In particular, when the goal is to build a convincing body of knowledge in a social program area about what works and why, it is important to consider how the experimental method can be adapted, extended, and supplemented by other approaches.

This paper illustrates recent efforts to do just that with social experiments involving American welfare-to-work and employment programs conducted by the Manpower Demonstration Research Corporation (MDRC), a New York-based social policy research firm. Many of the issues discussed in the paper will be explored further in a book being prepared by MDRC for the Russell Sage Foundation.

During the past 25 years, MDRC has conducted 30 major random assignment experiments involving nearly 300,000 people (Gueron, 1999). The collection of experiments described here—many of which are still in progress or just beginning—reveals an ongoing evolution in analytical approaches. Implicit in these new directions is a view that random assignment cannot stand alone, and that researchers and policymakers must look beyond the "experimental *versus* non-experimental" debate toward productive ways of combining both approaches.

The paper begins by reviewing the use of a simple random assignment design to test the overall effects of a welfare-to-work program. It then highlights the advantages and limitations of this method and illustrates how some of the limitations can be overcome by a multi-group random assignment design, which has been used by several recent studies. The paper next discusses new efforts to test a broader range of hypotheses concerning factors that drive program performance. These efforts involve, in one example, pooling data from multiple experiments and applying hierarchical linear modeling techniques to a comparison-of-sites analysis, and, in another case, using instrumental variables within the context of social experiments to test hypothesized causal relationships between a program's intermediate effects and its ultimate impacts. Finally, the paper discusses the mixing of cluster random assignment (random assignment of groups) and a quasi-experimental interrupted time-series design to study the effectiveness of a new employment initiative targeted on whole communities—specifically, public housing developments.

## Basic Randomized Designs That Test Programs in Their Entirety (Testing the "Black Box")

Simple randomized experiments to test social programs work much like medical clinical trials to test new drugs. Individuals are randomly assigned through a lottery-like process to either a program group, which is offered the new treatment, or a control group, which is not. This procedure ensures that the two groups are equivalent on all pre-random-assignment characteristics, both measured and unmeasured (assuming a sufficiently large sample) because each sample member has the same chance of being selected for the program. Thus, the subsequent outcomes of the control group accurately reflect what the program group's outcomes would have been in the absence of the program. Therefore, any observed differences between the two groups' future outcomes can be attributed to the program. For example, if 50 percent of the persons assigned to a welfare-to-work program found employment compared with only 40 percent of control group members, the 10 percentage point difference in their employment rates would represent the effect, or "impact," caused by the program.

Most experiments conducted on welfare-to-work or job-training programs test the program in its entirety instead of testing its separate components. Doing so is sometimes referred to as testing a "black box," not what is inside of it. An illustration of this approach is provided by MDRC's evaluation of California's Greater Avenues for Independence Program (GAIN).

GAIN was created by the California legislature in the mid-1980s to promote a more work-focused welfare system that balanced new opportunities for recipients with new obligations. The program was targeted on applicants for and recipients of Aid to Families with Dependent Children (AFDC), the national cash welfare system in the US at the time. The GAIN program offered a variety of work-preparation activities provided in specific sequences for different types of participants. Job-search assistance and basic education or English language instruction were the primary initial activities, followed, if appropriate, by vocational training, post-secondary education, or unpaid work experience in a public or not-for-profit organization. The program also provided financial assistance for child-care and other support services, plus assignment to a case manager. Welfare recipients who failed to participate in the program without "good cause" were to be "sanctioned" through a reduction in their welfare grant.

The GAIN model was the result of a political compromise among legislators, who debated how much the program should emphasize human capital development activities versus rapid employment, how much it should stress unpaid work experience, and whether participation in the program should be mandatory or voluntary. In the end, the new program stood in sharp contrast in scope, intensity, complexity, and expense to the job-search-only or job search/work experience programs of the early 1980s (Gueron and Pauly, 1991; Friedlander and Burtless, 1995). Given the many changes embodied in this new approach, lawmakers wanted to know, with confidence: "Does it make a difference"?

To answer this question, a randomized experiment was launched in six California counties (Alameda, Butte, Los Angeles, Riverside, San Diego, and Tulare) that represented much of the state's diversity in welfare populations and local labor markets. Within each county, welfare applicants and recipients with school-age children were randomly assigned to the GAIN program or to a control group that was excluded from the program. Control group members could seek alternative services in the community on their own, but were not required to do so. The core sample for the study included nearly 23,000 single parents (mostly mothers), about 22 percent of whom were assigned to the control group. The employment and welfare experiences of the two groups, along with other outcomes, were then tracked for five years after each sample member's date of random assignment using administrative records. Based on this follow-up information, program impacts were estimated separately for each county, thereby providing six large-sample tests of the GAIN model (Riccio, Friedlander, and Freedman, 1994; Freedman et al., 1996).

During the five-year follow-up period, GAIN produced statistically significant and substantively important impacts in each county. However, one county, Riverside, strongly outperformed the others by producing larger impacts on more variables across more subgroups of welfare recipients. At the opposite extreme, Los Angeles was distinguished by its lack of impacts on earnings.[1] Overall, though, the six county tests suggested that the GAIN model was fairly "robust" and thus could produce positive effects when operated by different staff, for different types of people, in diverse settings.

Still, the variation in county performance was substantial and the randomized experiment was less helpful with respect to understanding what caused the variation than it was in documenting its existence. To address this question, a detailed study of the variation in county GAIN program practices was undertaken, drawing on a rich body of field observations in local GAIN offices and an extensive survey of their staff perceptions and practices. (These variations in practices reflected the fact that California's welfare system allowed county officials a certain amount of flexibility in how they implemented state policies.) In addition, differences across counties in the characteristics of sample members were controlled for statistically, and the variation in local labor market conditions was examined.

This analysis suggested that Riverside's superior performance derived from its *combination* of practices rather than any single practice. Key features of these practices included: a pervasive emphasis on moving recipients quickly into the labor market, backed up by staff efforts to locate job openings; a relatively balanced use of job search and education activities for recipients needing remedial education; a commitment to working with all enrollees, not just the most job-ready; close monitoring of recipients' participation in the program; and firm enforcement of the program's participation mandate (Riccio, Friedlander, and Freedman, 1994; Riccio and Hasenfeld, 1996; and Riccio and Orenstein, 1996).

Although the assessment of cross-county variation in program performance was carefully done, considerable uncertainty remained because it was not possible to isolate the effect of specific factors, given the large number of factors that varied across the small number of counties involved. Nevertheless, the analysis cast doubt on the labor market benefits of GAIN's huge investment in basic education because those counties that increased participation in this component the most (relative to the control group) experienced some of the smallest program impacts that were observed.

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[1] In response to its own poor results and inspired by Riverside's impressive effects, Los Angeles County later revamped its program to emphasize rapid employment, in contrast to the strong education focus of its original program. Findings from a new randomized experiment indicate that the redesigned program produced substantial employment and earnings impacts for a broad range of subgroups during two years of follow-up (Freedman et al., 2000).

## Multi-Program Group Designs That Compare Program Strategies and Components (Prying-Open the "Black Box")

The policy debates in California over the structure of GAIN reflected many nationally relevant themes and controversies. Thus it was not surprising that new federal welfare legislation enacted in 1988 (the Family Support Act) fostered welfare-to-work programs with many features similar to those of GAIN. In addition this legislation created an opportunity for more rigorous tests of specific welfare-to-work strategies, not just whole programs.

Toward this end, the National Evaluation of Welfare-to-Work Strategies (NEWWS), sponsored by the U.S. Department of Health and Human Services, launched a series of social experiments in seven localities in six states. In four of these localities, policymakers agreed to conduct experiments with two different treatment groups plus a control group. This multi-program group design provided "head-to-head" experimental comparisons of alternative job preparation strategies (in three states) and alternative ways to structure program caseloads (in one state).

During the same period, the state of Minnesota launched an experiment that used a three-group randomized design to help isolate the effects of a financial incentive that was part of the state's new welfare-to-work strategy—the Minnesota Family Investment Program (MFIP). To illustrate the potential and limitations of these multi-program group designs, key features and findings of the NEWWS and MFIP experiments are described briefly below.

### Testing Human Capital Development Versus Labor Force Attachment Approaches

Randomized experiments in three NEWWS sites—Atlanta, Georgia; Grand Rapids, Michigan; and Riverside, California—tested two competing theories about how best to help individuals make a lasting transition from welfare to work: (1) by encouraging them to take a job (any job) quickly as a stepping stone toward long-term economic self-sufficiency, or (2) by inducing them to participate in education and training activities designed to increase their human capital and thereby improve their future economic prospects (see Hamilton et al., 1997).

To test these two theories, welfare applicants and recipients were randomly assigned to one of three groups at each site: (1) a "labor force attachment" group that emphasized rapid employment, (2) a "human capital development" group that emphasized education and training prior to employment, or (3) a control group that was not offered program services but was free to seek them from elsewhere in the community. The human capital strategy emphasized basic education as its initial activity for recipients with weak basic skills or limited prior education, and vocational training as its initial activity for recipients with a high school degree or its equivalent. In addition, the strategy provided job search assistance as a subsequent activity but did not emphasize it. In contrast, the labor force attachment strategy emphasized job search assistance as its first

and primary activity; the strategy only provided education and training when its first component was not successful.

Findings from the study's implementation research indicated that local program staff delivered—and recipients in the different program groups heard—quite different messages about how best to move from welfare to work. Moreover, a cost analysis revealed that the human capital development programs were about twice as expensive as the labor force attachment programs. Thus, there was a substantial difference between the two program options tested.

The three-group randomized design used by these sites provided unbiased estimates of three types of impacts: (1) the *net* impacts of the labor force attachment strategy (from the difference in outcomes for the first program group and the control group), (2) the *net* impacts of the human capital development strategy (from the difference in outcomes for the second program group and the control group), plus (3) the *differential* impacts of the two strategies (from the difference in outcomes for the two program groups).

Interim NEWWS findings for the first two years after random assignment indicate that both program strategies had positive net impacts on total two-year earnings (Hamilton et al., 1997). However, the impacts of the labor force attachment strategy tended to be larger than those of the human capital strategy. This may be due, in part, to the fact that a number of human capital group members were in school (and thus not employed) for a portion of the two-year follow-up period. At the present time, five years of NEWWS follow-up data have been collected but the analysis of these data is not complete. Thus, it is not yet clear whether the short-term superiority of the labor force attachment approach will persist over time.

**Testing Integrated Versus Specialized Case Management**

A fourth NEWWS site—Columbus, Ohio—used a three-group randomized design to test alternative ways of structuring case management in welfare-to-work programs. Case managers in such programs must become employment specialists who perform a variety of tasks to help welfare recipients get and keep jobs. These tasks typically include, among others: assessing client employment barriers, guiding them toward specific program activities, arranging family support services, monitoring individual participation, and, when necessary, initiating financial sanctions for lack of program participation. Traditionally, these functions are kept separate from those of income maintenance workers, who must authorize and process welfare checks, determine program eligibility, and implement financial sanctions when notified to do so.

Proponents of separating the income maintenance and employment functions of welfare-to-work programs argue that doing so allows staff to specialize in a narrow range of functions which may call for different skills. This specialization could in turn, foster more efficient and higher-quality performance of each set of functions. On the other hand, critics of separating these functions argue that doing so slows the movement of

welfare recipients into work-promoting activities and makes it tougher for state and local administrators to change the prevailing culture of the welfare system from its focus on "processing people and paper" to one that emphasizes helping recipients to become employed. From this perspective, integrating the different case management functions and assigning both to the same staff may be more effective.

To help inform this debate, the Columbus, Ohio NEWWS site conducted a social experiment that compared these two approaches in the context of a human capital oriented welfare-to work program (Brock and Harknett, 1998; Scrivener, Walter, Brock, and Hamilton, 2001). For this study, welfare staff were divided into two groups: an *integrated group* whose members performed both income maintenance and employment functions, and a *specialized group* (referred to as the "traditional" group in the original study) whose members performed either income maintenance functions or employment functions, but not both. Welfare recipients were then randomly assigned to one of these two groups or a control group that was not eligible for the program.

The findings from this study, based on three years of follow-up data, indicate that the integrated approach produced higher rates of program participation, although both approaches had modest positive impacts on earnings. The integrated approach produced somewhat higher welfare savings, perhaps because case managers with integrated functions discovered program noncompliance and imposed financial sanctions (thus reducing welfare payments) more quickly than did those with separate functions.

**Isolating the Effects of Financial Incentives**

The preceding examples illustrate how differential impact experiments can be used to determine whether one treatment strategy is better than another. This design also can be used to help untangle the effects of components of a multifaceted strategy.

One recent such application is the MDRC evaluation of the Minnesota Family Investment Program (MFIP), an innovative welfare-to-work program that combines mandated participation in employment and training services with a special financial incentive to work. The incentive was designed to "make work pay more" by allowing welfare recipients who became employed to keep more of their welfare benefits than would have been possible otherwise. An important question for policymakers in this context is whether the enhanced work incentive is sufficient by itself to markedly increase employment and earnings among welfare recipients or whether additional services are required to do so.

To answer this question, individuals were randomly assigned to either: (1) a program group that was offered only the new incentive, (2) a second program group that received the full MFIP treatment, which included both the new incentive offer and, when families had received cash welfare payments for 24 out of the past 36 months, the requirement to participate in MFIP's employment and training services, or (3) a control group, that was eligible for neither MFIP's incentive or services but could, if they met certain criteria, volunteer for alternative services (see Miller et al., 2000). By comparing

subsequent outcomes for the incentives-only group with those for the control group, it was possible to estimate the *net* impacts of the financial incentive by itself. By comparing outcomes for the full-program group with those for the control group, it was possible to estimate the *net* impacts of the full program. Lastly, by comparing outcomes for the incentives-only group with those for the full-program group, it was possible to estimate the *incremental* impacts of adding MFIP services to the incentive. These findings could then be compared to corresponding estimates of program and component costs.

The results, which cover 30 months after random assignment, are most striking for long-term urban welfare recipients who were single parents. They indicate that: (1) the financial incentive offer, *by itself,* did not increase average earnings relative to the control group, although it did produce a small gain in the average rate of employment; (2) the full program increased both employment and earnings relative to the control group, and (3) both the incentive offer alone and the full program caused welfare receipt to increase relative to the control group, but the full program did so to a much lesser extent, indicating that the mandates and services it provided offset some of the effects of the financial incentive on welfare receipt (Miller et al., 2000).

**Limitations of Differential Impact Designs**

The three-way random assignment designs used for the preceding examples represent a powerful way to compare alternative treatments or to get part way inside the black box of a multi-faceted program. However, these designs are limited by the fact that most social programs are made up of a variety of components, features, or strategies while, in most cases, it is only feasible to implement large-sample randomized social experiments with a small number of program groups. Thus, in practice, it usually is only possible to test a very small number of program components or alternative program approaches at a time. Moreover, alternative approaches often comprise multi-dimensional "packages" of features and thus also represent black boxes that, although more narrowly defined, must be unpacked themselves.

## Pooling Experimental Data in a "Comparison-of-Sites" Analysis (Linking Program Impacts and Implementation)

As social experiments in the same field accumulate over time, new opportunities become available for combining experiments in order to test a broad range of hypotheses about what drives program performance. Specifically, *if comparable data are collected* across experimental studies and across sites within these studies, it is possible to use the natural variation in true impacts across sites (estimated by an experiment for each) and the natural variation in their programs, sample members, and environments to estimate the influence of each factor on program impacts, controlling for the others. (This comparison-of sites design is described in detail by Plewis, 2001, in the present volume, as is the use of hierarchical modeling to estimate the relationships involved. For a more general and detailed discussion of hierarchical modeling, see Bryk and Raudenbush, 1992.)

The viability of this approach depends on the number of experimental sites for which comparable data have been obtained (which can be increased by pooling data across studies), the variability of true impacts across sites (the dependent variable for the analysis), the size of the experimental sample at each site (which determines the precision of site-specific program impact estimates), and the quality and consistency of measures used to characterize programs, sample members, and site environments (the independent variables for the analysis).

MDRC has just completed the first stage of a study that applies this comparison-of-experimental-sites approach to examine the effects of program management, program services, the local economy and client characteristics on the impacts of welfare-to-work programs in a wide range of locations across the US (Bloom, Hill, and Riccio, 2001). The study pools original data from three large-scale, multi-site randomized experiments conducted by MDRC: the GAIN and NEWWS evaluations discussed earlier plus a similar evaluation of Project Independence in the State of Florida (Kemple, Friedlander, and Fellerath, 1995). The pooled sample comprises 69,399 program and control group members from 59 local program offices (sites).

In contrast to the original evaluations, which focused on each program's overall employment, earnings and welfare impacts on *individuals*, the new study focuses on the variation in impacts across welfare-to-work *offices* that administered the programs and the factors that produced this variation. This is accomplished by estimating a two-level hierarchical linear model.

The first level of the model specifies individual outcomes (total earnings during the first two years after random assignment) as a function of individual background characteristics, program/control group status and a series of interactions between background characteristics and program/control status. The output of this level is a program impact estimate for each local office that controls statistically for the background characteristics of its sample members. Such findings are often referred to as "conditional" impact estimates. The second level of the model specifies the conditional impact for each office as a function of how its program was managed, the extent to which it increased the use of specific employment and training services, and its prevailing local unemployment rate. Although the model is specified as two separate levels, all of its parameters are estimated simultaneously and each parameter represents the effect of a specific individual-level or office-level variable holding constant all other variables in both levels of the model.

The office-level *program management measures* are scales (usually based on multiple items) constructed from responses to local office staff surveys conducted for the original evaluation studies. Almost all local staff in the original programs responded to these surveys and, on average, there are 21 respondents per office. From these responses, measures of the following variables were constructed: program emphasis on quick client employment; program emphasis on personalized client attention; program emphasis on close client monitoring; the differences between line staff and their immediate

supervisors in the emphasis placed on this set of program practices; the variation among the line staff themselves in the emphasis placed on these practices; and average caseload size per staff member.

Office-level *program service measures* were obtained from follow-up surveys administered for the original evaluations to a subsample of program and control group members from each local office; follow-up survey samples averaged 258 respondents per office. From this information it was possible to compute separately the *difference* in the percentage of program group and control group members who received basic (remedial) education, job-search assistance, and vocational training.

Lastly, the prevailing unemployment rate for each office was obtained from publicly available sources and included in the model to represent *local economic conditions*.

Key findings from the analysis (which were subjected to an extensive series of sensitivity tests and found to be quite robust) are that: (1) A strong *employment message emphasizing quick job entry* is a powerful medium for stimulating clients to find jobs; (2) staff emphasis on *personalized client attention* markedly increases program success; (3) *large caseloads* reduce program effectiveness; (4) increased reliance on *basic education* reduces program impacts, at least in the short-term; and (5) *high unemployment rates* reduce program impacts.

These findings address major questions about welfare-to-work program design and operation and bear directly on hypotheses that have been debated for decades by practitioners and researchers alike. Hence, the findings have potentially major substantive implications for future welfare policies and programs. Furthermore, they help to illustrate what can be accomplished by pooling comparable data from a series of high-quality randomized experiments conducted in different locations and at different times. Therefore the study also has important methodological implications.

However, when assessing the comparison-of-experimental-sites approach, it is important to acknowledge that, even though evidence of each site's performance is based on a randomized experiment, all relationships between program impacts and their hypothesized determinants are estimated from a cross-sectional non-experimental analysis (the second level of the hierarchical model). Hence, these findings may be subject to the same potential for selection bias due to incomplete model specification that is inherent in any non-experimental research. The best ways to reduce the potential for such problems is to: ground the statistical model to be used as fully as possible in the theory and practice of the phenomenon to be studied; carefully develop and test the validity and reliability of all measures used; and obtain a sample of sites and individuals that is as large and varied as possible. It is hoped that by pooling a series of multi-site experiments that were conducted in similar ways using comparable data that was rich in scope and based closely on widely-held (and debated) theories and hypotheses, that the present MDRC study has taken a meaningful step in this direction.

## Using Instrumental Variables with Randomized Experiments (Exploring Causal Pathways)[2]

One frequent complaint about randomized experiments is that they pay too little attention to the underlying theories of the programs they are designed to test. Thus, they have not contributed as much as they could have to the understanding of social problems (Chen and Rossi, 1983). To remedy this problem, what is needed, many argue, is a clear specification of the causal pathways by which impacts are expected to occur and a method of analysis that can test for the existence and strength of the intervening steps in these pathways (Weiss, 1995). Unfortunately, the effects of these intervening variables—also called mediating variables, or "mediators"—are difficult to estimate.

One promising approach for doing so is the method of instrumental variables. This approach has been well known for many years and is described in many textbooks (Greene, 1997). However, the assumptions required for the approach to provide valid results are difficult to meet in practice, and, hence, it has not played a major role in the development of program theory. Recently, however, there has been a resurgence of interest in the method, growing out of some innovative applications that use "natural experiments" to generate the exogenous variation required for the method to work (Card, 1995; Angrist, Imbens, and Rubin, 1996). Because exogenous variation also can be generated—by design—through random assignment experiments, the use of instrumental variables is now being considered within the context of such experiments as a way to study the effects of mediating variables.

The basic logic of this approach is as follows. Consider a hypothetical welfare-to-work program for adults that provides subsidized child care to parents of young children, and assume for a moment that the only way the program can affect child behavior is through its impact on the use of child care (Figure 1a). In other words, assume that child-care is the only mediating variable in this relationship. Thus, the program impact on child behavior equals the *product* of its effect on the use of child-care times the effect of child care on child behavior. This relationship provides a simple way to use instrumental variables in the context of a random assignment experiment to estimate the effect of child-care on child behavior.

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[2] This section was adapted from an unpublished MDRC document prepared by Howard Bloom, with assistance from Hans Bos, Lisa Gennetian, and Pamela Morris.

**Figure 1**
**Illustrative Models of Mediating Variables for Interpreting Program Impacts**

**Figure 1a**

**Figure 1b**

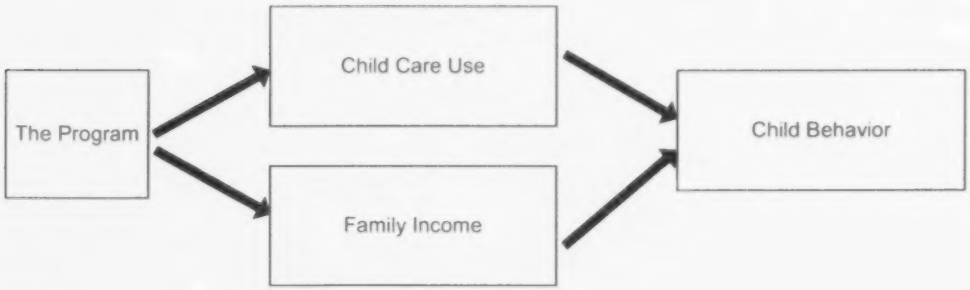
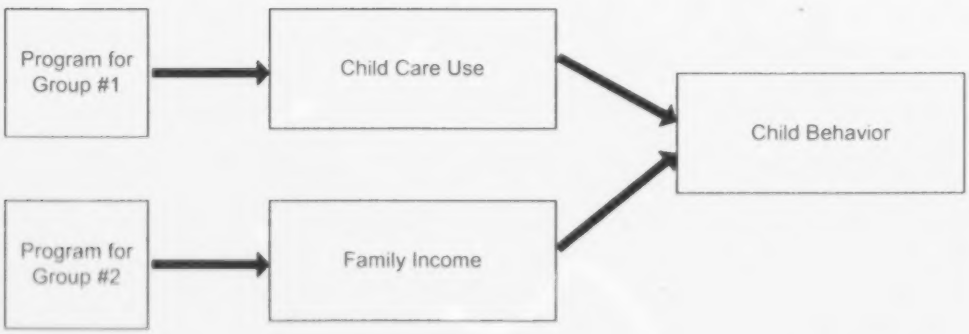



**Figure 1c**



To see this, note that valid estimates of two of the three elements of the relationship (program impacts on child-care use and on child behavior) can be obtained directly from an experimental design by comparing program and control group means for each outcome. Because of this, a valid estimate of the effect of child-care on child behavior can be obtained by *dividing* the estimated program impact on child behavior by the estimated program impact on child-care use. Doing so is equivalent to using the randomly assigned program status of individual sample members as an instrumental variable to estimating the relationship between child behavior and child-care; standard errors for which can be obtained readily from many software packages.

Applications of this approach to different evaluation research questions have been explored by MDRC (Bos and Granger, 1999; Morris and Gennetian, 1999; Boudett and Friedlander, 1997; and Bloom, 1984) and others (Ludwig, Duncan, and Hirschfield, 1998). One problem that has been encountered by these applications is limited statistical power due to weak program impacts on the mediator. In other words there is often a weak connection between the independent variable of interest (use of child-care in the present example) and its "instrument" (random assignment status). This is a common problem with instrumental variables methods in both experimental and non-experimental analyses (Bound, Jaeger, and Baker, 1995). To address this problem, it is necessary to identify experimental groups with large and statistically significant program impacts on the mediator of interest. MDRC is thus examining past and current experiments to identify empirical examples where this condition holds.

A second problem that has been encountered in applications of the approach is potential bias due to the existence of more than one mediator (causal pathway) for a program impact. Figure 1b illustrates this situation for a hypothetical welfare-to-work program that affects both child-care use and family income, each of which affects child behavior. In this case, there is no simple way to separate the effect of the two mediators on child behavior. If one mediator were ignored and the instrument were applied to the other, the full impact of both would be attributed to the one included. If both mediators were included in the analysis, the model would not be "identified" and the effects of neither mediator could be estimated.

Figure 1c illustrates an approach to dealing with the problem of multiple mediators that is being explored by MDRC—the identification of sites, subgroups or different treatment streams with a large program impact on only one mediator, with different groups having impacts on different mediators. If, for example, the program at one site had a large impact on the use of child care but not on family income (perhaps because the program focused mainly on providing child care and did little to help participants get jobs) and if there were no other plausible connection between the

program for adults and outcomes for their children, then random assignment status at this site could be used as an instrument to estimate the effect of child care on child outcomes. If, at another site, the situation were reversed—the program had a large effect on family income and not on child-care use—then random assignment status at that site could be used as an instrument to estimate the impact of family income on child outcomes. Combining the results for the two sites could thus, facilitate a comparison of the impacts of the two mediators.

At this point, several initiatives to explore these issues in the context of welfare and employment programs are underway at MDRC. In addition, other researchers are exploring applications of the approach in other settings. Although the outcomes of these efforts remain an open question, there are ample reasons for expecting them to provide new avenues for using randomized experiments to deepen our understanding of social problems and to provide insights into how programs and policies can better address these problems.

## Combining "Cluster" Random Assignment with Interrupted Time-Series Analysis (Testing "Place-Based" Initiatives)

Recent years have seen a marked increase in comprehensive community initiatives designed to improve neighborhoods with high concentrations of poverty (Fulbright-Anderson, Kubisch, and Connell, 1998). In addition, there have been several major attempts to focus public health initiatives on specific geographic areas (Murray, Hannan, Jacobs, McGovern, Schmid, Baker, and Gray, 1994). Concurrently, there have been a large and growing number of attempts to implement whole school reforms (Bloom, 2001). These initiatives share a very important common feature: they focus on whole groups, not on specific individuals.

Because of this, attempts to evaluate the initiatives share a number of common problems (Hollister and Hill, 1995; Rossi, 1999). At the root of these problems is the fact that many factors in addition to a program can spark community-wide, locality-wide or school-wide changes. Such factors include among others: population mobility, changes in economic or social conditions, and concurrent changes in related public policies. These factors make it very difficult to isolate a specific intervention's contribution to observed change. Furthermore, because such interventions are targeted on whole groups, it is usually not feasible to randomly assign individuals to program or control status.

In some cases it is possible to use whole communities, localities, or schools (aggregate units) as the basis for analysis by randomly assigning some to the intervention and others to a control group (Boruch and Foley, 1999). This approach is referred to as "cluster" random assignment (Bloom, Bos, and Lee, 1999). However, because the approach can be difficult and expensive to implement—especially for a large number of aggregate units—it is not feasible to rely *solely* on it for measuring the effectiveness of many group-focused programs. Thus, for such programs, it is important to consider alternative approaches that combine experimental and quasi-experimental methods.

MDRC is currently using such a combined strategy for a research demonstration project called The Jobs-Plus Community Revitalization Initiative for Public Housing Families (Riccio, 1999). Jobs-Plus is being sponsored by The Rockefeller Foundation, the U.S. Department of Housing and Urban Development, and several other federal agencies and private foundations. The goal of the project, which is very ambitious, is to increase dramatically the employment rates and earnings of residents living in government-owned housing developments (housing estates) and thereby transform low-work, high-welfare developments into high-work, low-welfare communities.

At the heart of the Jobs-Plus approach are three core components: (1) state-of-the-art employment-related activities and services, (2) enhanced financial incentives to work (a reduction in the rent increase that occurs when residents go to work, and thus increase their earnings), and (3) "community support for work", which entails, among other things, work-related information-sharing, peer support, and mutual aid among residents. Jobs-Plus is distinctive both with regard to its explicit combination of these approaches and with regard to its targeting of them on *all* working-age residents of public housing developments in the program. In other words, it attempts to "saturate" the targeted environment. It is hoped that this "saturation" approach to the provision of services, incentives, and social supports will make it possible and profitable for a substantial majority of working-age public housing residents to become employed steadily.

The project was launched in seven cities, five of which (Baltimore, Maryland; Chattanooga, Tennessee; Dayton, Ohio; Los Angeles, California; and St. Paul, Minnesota) are currently part of an impact evaluation study. Underlying the program model for this study is a theory positing that big changes in employment can only be achieved by transforming social norms and relationships within each development, which, in turn, requires reaching out to all working-age residents.

Because it was not possible to randomly assign individual residents to a program or control group, an alternative design is being used to measure the impacts of Jobs-Plus (Bloom, 1996). This design combines random assignment of selected public housing developments in each city to a program or control group (cluster random assignment) with a well-known quasi-experimental approach called "interrupted time-series analysis."

The first step in the design was for participating cities to nominate several public housing developments with roughly similar demographic characteristics (thereby matching these developments to some extent). Then for each city, one nominated development was randomly chosen to operate the Jobs-Plus program, and one or (more often) two were chosen for a control group. This random allocation of public housing developments was an attempt to avoid selection bias that would have resulted if MDRC had systematically picked the "strongest" or "weakest" housing developments for the program.

Thus, when findings for all cities are pooled, subsequent differences in program and control group outcomes will provide unbiased estimates of program impacts, as is the

case for any randomized experiment. However, the small number of housing developments that were randomly assigned limits the methodology in a very important way. With very few developments, it is possible that by chance, those randomly assigned to the program group will have a proportion of job-ready residents that is higher (or lower) than that of developments randomly assigned to the control group. Thus, future differences in outcomes for the two groups, while not biased *per se* (because every development in each city had the same probability of assignment to the program) are not accurate estimates of the impacts caused by the program. If, on the other hand, many more developments had been available for random assignment, the potential for a non-comparable program and control group would be much less, and, hence, program impact estimates based on observed future outcome differences would be much more accurate.

This relationship between the number of aggregate units available for random assignment and the likely accuracy of program impact estimates reflects the usual role of random error in statistics. For small samples the influence of such error is large and for large samples its influence is predictably small. Thus, as in any other statistical analysis, small samples have low statistical power (their standard errors are large) and large samples have high statistical power (their standard errors are small). In the case of cluster random assignment, the statistical power of program impact estimates can depend far more on the number of aggregate units than on the number of individuals within each aggregate unit (Raudenbush, 1997).

Therefore, standard pooled experimental estimates of Jobs-Plus impacts most likely will have low statistical power (and thus, large standard errors), even though developments from each city were matched prior to random assignment in order to limit their initial disparities (and existing demographic and baseline survey data suggest that these disparities typically are small). This suggests that relying solely on experimental impact estimates would not be an appropriate analytic strategy for the Jobs-Plus evaluation. Further reinforcing this conclusion is the fact that experimental impact estimates for a given city are not possible because only one development per city was randomly assigned to the program.

To address these limitations, the Jobs-Plus impact evaluation also incorporates features of a quasi-experimental design, which takes the form of a comparative interrupted time-series analysis. Interrupted time-series analysis has been used for decades to evaluate many different programs and policies (Campbell and Stanley, 1966; Cook and Campbell, 1979; Shadish, Cook and Campbell, forthcoming, 2001). At its core, the design represents an extension of "before-after" analysis with multiple observations on outcomes before an intervention is launched (during its baseline period) and multiple observations after it is launched (during its follow-up period). In its simplest form, interrupted time-series analysis estimates program impacts for a given follow-up period as its deviation from the baseline pattern for an outcome of interest. There are many ways to characterize the baseline outcome pattern and many ways to characterize how follow-up outcomes depart from this pattern. Nevertheless, in all cases, program impacts are estimated in terms of *follow-up deviations from baseline patterns*.

The primary outcomes for the Jobs-Plus evaluation are employment, earnings and welfare receipt. Quarterly data on employment and earnings will be obtained from the administrative records of state Unemployment Insurance (UI) agencies for a baseline period of five years before Jobs-Plus was launched in each program development and a follow-up period of three to five years thereafter (Kornfeld and Bloom, 1999, find that UI wage records are good source of such data). Likewise, monthly data on the receipt of welfare benefit payments (for AFDC and food stamps) during the same period will be obtained from the administrative records of state or local welfare agencies. From this information, it will be possible to observe how each outcome at a given program development deviated from its baseline pattern. Thus, for example, it will be possible to track progress during the follow-up period in the percentage of residents who are employed, their mean earnings, the percentage of residents who are receiving public welfare benefits (by source), and the mean value of benefits that they receive.[3] This progress will form the basis for estimates of Jobs-Plus impacts.

The next part of the estimation strategy involves a corresponding interrupted time-series analysis of how outcomes changed at the public housing developments in the control group during the same time period. Thus, the follow-up deviation from the baseline pattern in the control group for each city can be used to predict what the corresponding deviation would have been for its program development if Jobs-Plus had not been launched. Hence, the control group deviation from its baseline pattern provides an estimate of the counterfactual for each program development that helps to control for other major national, statewide or local changes that occurred after Jobs-Plus had been launched (such as welfare reform in the US and changes in the US economy). Thus, for each site, the impact of Jobs-Plus will be estimated as the difference between the program and control development deviations from their own baseline patterns.

This approach makes it possible to estimate the impacts of Jobs-Plus for each site. In addition, it provides estimates of the overall average impact of the program that have the methodological protection of the interrupted time-series design for each site and the methodological protection of cluster random assignment for pooling findings across sites. Thus, the combination of experimental and quasi-experimental elements makes the Jobs-Plus evaluation design a promising candidate for assessing the effectiveness of a community initiative.

Nevertheless, in principle, this design is weaker than a classical social experiment that randomly assigns large numbers of individuals to a treatment or control group. Thus, for studies of programs that require an ability to precisely identify small impacts with confidence, the approach may not be adequate. However, the explicit goal of Jobs-Plus is to produce *large* impacts. Thus, from a policy perspective, it will not have succeeded if it only produces small effects; Therefore knowing whether small impacts are "real" is not

[3] The analysis will estimate impacts at both an individual and a development level of analysis. The former will assess the impacts on an original cohort of residents living in the developments just prior to the start of Jobs-Plus; they will be tracked whether or not they continue living in the developments. The latter will focus on impacts for the developments as a whole, regardless of the year-to-year turnover among the particular people living there by measuring outcomes for persons who lived in the developments each year (see Bloom, 1999).

essential. If the estimated impacts are large, this may be sufficient for concluding with confidence that Jobs-Plus indeed made a difference.

## Conclusions

This essay has described the evolution of social experiments used to evaluate welfare-to-work and employment programs. These experiments have developed from simple "black box" designs that measure only the "net" impact of an overall program to more complex multi-group designs that measure the "differential" impacts of alternative program strategies or the "incremental" impacts of adding program elements to each other. In addition, evaluators have begun to augment social experiments with non-experimental and quasi-experimental strategies. Many of the studies reviewed above are still underway or just beginning, so the full contribution of the methods they employ remains to be seen. However, we believe that they reflect an important push in new directions that ultimately will extend the reach of social experiments and thus increase their contribution to knowledge.

Thus, future evaluation research could do well to continue down this path. In some cases, this means designing more sophisticated social experiments from the start. In other cases, it means envisioning the potential scientific value of pooling data from different social experiments and ensuring that the common types of information necessary to do so are collected. In yet other cases, it means looking within experiments for opportunities presented by the pattern of findings that can be exploited to test important hypotheses (as in the use of instrumental variables). Furthermore, it also means thinking creatively about ways to combine experiments with other methods in order to address important evaluation questions that cannot be studied by classical experimental methods alone.

In closing, it is important to note that the debate over the advantages and disadvantages of experimental and non-experimental methods is longstanding and vigorous and remains important. However, dwelling on the likely superiority of one approach over the other is likely to be counterproductive. Instead of seeking to inspire wholesale acceptance or rejection of one set of methods over the other, evaluation researchers should engage in a more intensive search for new ways and opportunities to use both productively.

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*Building a Convincing Test of a Public Housing Employment Program Using Non-Experimental Methods: Planning for the Jobs-Plus Demonstration*. 1999. Howard Bloom.

*Estimating Program Impacts on Student Achievement Using "Short" Interrupted Time Series*. 1999. Howard Bloom.

*Using Cluster Random Assignment to Measure Program Impacts: Statistical Implications for the Evaluation of Education Programs*. 1999. Howard Bloom, Johannes Bos, Suk-Won Lee.

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The Manpower Demonstration Research Corporation (MDRC) is a nonprofit, nonpartisan social policy research organization. We are dedicated to learning what works to improve the well-being of low-income people. Through our research and the active communication of our findings, we seek to enhance the effectiveness of social policies and programs. MDRC was founded in 1974 and is located in New York City and San Francisco.

MDRC's current projects focus on welfare and economic security, education, and employment and community initiatives. Complementing our evaluations of a wide range of welfare reforms are new studies of supports for the working poor and emerging analyses of how programs affect children's development and their families' well-being. In the field of education, we are testing reforms aimed at improving the performance of public schools, especially in urban areas. Finally, our community projects are using innovative approaches to increase employment in low-income neighborhoods.

Our projects are a mix of demonstrations — field tests of promising program models — and evaluations of government and community initiatives, and we employ a wide range of methods to determine a program's effects, including large-scale studies, surveys, case studies, and ethnographies of individuals and families. We share the findings and lessons from our work — including best practices for program operators — with a broad audience within the policy and practitioner community, as well as the general public and the media.

Over the past quarter century, MDRC has worked in almost every state, all of the nation's largest cities, and Canada. We conduct our projects in partnership with state and local governments, the federal government, public school systems, community organizations, and numerous private philanthropies.